SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES IN THE MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Structure of Erectile Tissues.*—In our Number of November, 1835, p. 179, we published a detailed account of the discoveries of Professor Müller relative to the minute structure of the penis, the substance of which was that there exists a separate series of minute arteries (which, from their form, he named *helicine*) projecting into tho venous cells, and producing erection of the organ by the increased flow of blood through them, under circumstances of nervous excitement. His investigations were certainly of great importance, not only as establishing the minute structure of the organ described, but in a general point of view, as relating to a distribution of vessels to which no similar arrangement had been presumed to exist in the animal body—viz, arteries terminating with free extremities, in cells communicating with veins. We have now, however, to notice a complete refutation of the views then advanced, and since almost universally received, which has been published in the last number of Müller's *Archiv für Anatomie und Physiologie.* It is from the pen of Professor Valentin, the well-known author of the Entwickelungsgeschichte, whose talent for minute observation is certainly not inferior to that of Müller himself.

He says that the result of numerous examinations has convinced him that the so-called helicine arteries are by no means peculiar vessels, terminating with their extremities, and hanging free in the cells of the corpus cavernosnm, but only minute arteries that have been divided or torn; and that, on the contrary, the real distribution of the vessels of the corpus cavernosum follows in every respect the most simple laws. In making the injections of tho penis, different portions of it receive different quantities of injection; in general the posterior half is most injected, and of this the anterior fourth is best adapted for examination, because in it the injection will probably have exactly filled the minute arteries without passing into the venous cells. If a transverse section of a portion thus injected be made, one sees on its surface, together with arteries of various sizes running tortuously in the uninjured fibrous cord-like partitions* of the cells of tho corpus cavernosum, the *helicine arteries*—that is, arteries which, to the naked eye, or with a lens, seem to terminate suddenly, either singly or in tufts, which lie partly on tho fibrous cords, but principally hang loose in the cells, and which when placed in water appear fixed at one end, while the other floats out in it. They all, even to the naked eye, appear completely inclosed by a membrane exactly like the tissue of the partitions; and if they are examined with a micro-

* *Balken*, beams: the fibrous cords or bands which bound and traverse the so-called cells of the spleen.

scope, their ends appear sometimes ronnded, sometimes obliquely or nnevenly truncated, sometimes granular or even irregular; in a word, so inconsistent is the form of their terminations, as at once to suggest the opinion that they are unnaturally formed.

If a cleanly cut transverse section be examined with a good lens, with which a view to some little depth may he obtained, it will at once appear that there are helicine arteries only at the surface, and in the cells lying near it; but that in those cells which lie deeper, no trace of them can be seen. At the same time it may be remarked, that every fibrous cord, without exception, contains an artery of proportionate size, which runs in it tortuously, or rather in the form of a cork-screw; and that these arteries, like the fibrous cords in which they lie, communicate together. If the surface of the section be examined under water it will be seen that at the divided extremity of each fibrous partition, one or more helicine arteries seem to be given off, according as one or more smaller fibrous cords are given off together or separately from the chief one. These smaller fibres, when they were divided, had separated and contracted a little, and thus, and by their naturally winding course, the tendril-like or crozier-like terminations of the supposed helicine arteries were produced. Thus one sees how the helicine arteries are formed under one's own eyes. And wherever the minute arteries are filled with injection they may be made to appear helicine by dividing the fibrous cords in which they lie. In a longitudinal section the same thing may be observed, only that here still more partitions being divided, more helicine arteries aro seen; and more still may be made by cntting the corpus cavernosum, as one would with a saw; or hy washing out the injection from the cells into which it has run from the arteries, and so tearing a greater number of the extremely minute cords. By the careful examination of several of the arteries, and their fibrous cords supporting them, which are thus divided, a sufficient proof may be obtained that the apparent enlargement of their extremities, the closeness of their orifices, and their tortuous or tendril-like course, depend merely on the mode in which the section has been made, or on some artificial means employed in the examination.

In the posterior part of the corpus cavernosum in man the cells are large, and the fibrous cords traversing them very delicate, so that as all the minute arteries run tortuously on them, the belicine arteries seem to be very abundantly and evidently present. But more anteriorly, where these cells assume a more honey-comb appearance, and the fibrous partitions are more band like than cord like, and the arteries running on them are proportionally much smaller than the membranes surrounding them, the belicine appearance cannot be demonstrated. The most easy refutation of the presence of tho so-called helicine arteries is fonnd in the human species (in which it will be remembered, Müller said they could be most easily demonstrated,) and next to it, in that portion of the corpus spongiosum urethræ of the horse and ass, which immediately surrounds the urethra.—*London Medical Gazette*, June 16, 1838.

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2. *On the Capillary vessels.*—These vessels examined in many parts of the body, present, according to Dr. Krause of Hanover, a diameter considerably less than that of the smallest globule of the blood. He gives the following measurement of the diameter of the most delicate capillary vessels in various parts:

In the retina	$\frac{1}{340}$	of a line.
choroid	$\frac{1}{361}$	—
pnlmonary cells	$\frac{1}{361}$ $\frac{1}{398}$	—
intestinal follicles . . .	$\frac{1}{333}$	—
mnscular integument of small intestines,	$\frac{1}{740}$	—
the tibialis anticus muscle .	$\frac{1}{310}$	—

In proportion to the capillary vessels of the ordinary diameter, (i. e. $\frac{1}{300}$ to $\frac{1}{257}$ of a line,) these very delicate ones are always fewer in nnmber, and are generally placed intermediately between two larger branches. Krause has never found that the larger quantity of any capillary tissue was formed by these extremely

delicate vessels. The injections employed were vermilion and size, or the successivo injection of a solution of neutral chromate of potass, and acetato of lead, with some mucilage of gum arabic: the granules of chrome yellow thus formed are of a diameter of from $\frac{1}{800}$ to $\frac{1}{1200}$ of a line. The granules which remain after human blood has been macerated for two days in distilled water, (the kernels of the globules,) have a diameter of $\frac{1}{1600}$ of a line; but, on account of the feebleness with which they intercept the light, they are seen with more difficulty than some bodies of a smaller size. Contrary to this former opinion, the author is now convinced from observation that the capillary vessels have membranous parietes. *B. and F. Med. Rev.* from Müller's *Archiv. fur Anat. Physiol. und Wissent. Medicin.* Heft. i. 1837.

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3. *Thymus Gland.*—Dr. Krause opposes the notion that the thymus gland is not found after twelve years of age. He has found it in almost all individuals between twenty and thirty years; and very often larger than in young children; and ho has seen it of considerable size between the ages of thirty and fifty, and has also met with the brownish red remnants of it still later in life. In younger men its form is generally cleft in two parts, as in its original condition: these are generally adherent in tho middle only by cellular tissue, so that their decrease appears to commence at this part. The lower cornua never, as in children, descend to the upper part of the pericardium, but frequently extend far into the neck.

The following is the measurement of the thymus gland in some very healthy and well made individuals who had committed suicide:

Age and Sex.	Length.	Breadth.	Thickness.	Weight. Grains.	Volume. Cubic Inches.	Specific grav.
25, m.	34 lines.	18-25 lines,	4 lines.	292.5	0.977	1.0352
25, m.	42 —	32 —	2-3 —	380.3	1.156	1.0311
20, m.				356.5	1.085	1.0309
28, f.	22 —	16 —	2 —	69.2	0.211	1.0267

Ibid.

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4. *Intestinal Glands.*—Dr. Knause has carefully repeated the observations of Böhm on the intestinal glands (see this Journal for November 1837, p. 218,) and is quite satisfied of their accuracy. He adds some farther investigations, chiefly respecting the size of the various follicles and cripts of the intestinal canal. He does not, however, regard the glands of Peyer as essentially different from the solitary glands in the jejunum. The manifest differences are, that the glands of Peyer are crowded together and are naked and prominent on the free surface of the mucous membrane, whilst the solitary glands are abundantly beset with follicles. Both are of the same size, between $\frac{1}{4}$ and $\frac{4}{5}$ of a line, their cavity is rather more than half the size of their external circumference, the parietes proportionally thick; the contents of both are not readily squeezed out, but its appearance is that of opaque mucus, the granules of which, flattened and irregularly rounded, are from $\frac{1}{330}$ to $\frac{1}{250}$ of a line in diameter; a size which corresponds with that of the granules of the mucus of many mucous membranes, though they are occasionally found of a larger size. But both the solitary glands and those of Peyer present a character essentially different from that of the glands of other mucous membranes, i. e. a slight roughness of the internal surface of their cavities, produced by a slight prominence of secreting cells, and a plurality of openings, whilst the glands of mucous membranes generally possess but one opening connected with their cavity. It is very rare to find but one opening to either the solitary glands or those of Peyer, the average number of their apertures being from five to ten. The openings traverse the parietes of the follicles in an oblique direction, and it is consequently difficult to assure one's self that they communicate with their cavities. The evidence of this, however, may be obtained thus: Take a large follicle of a gland of Peyer, and open its cavity by removing that half of it which is inserted into the submucous cellular tissue of

the intestine; into tho little rounded hollow, which is tbns rendered visible, insert a very small drop of carmine in solution. On examining the opposite part of the follicle, i. e. that which projects into the intestinal canal where the openings are situated, the red fluid will be seeu to escapo from these apertures before tbo entire follicle becomes coloured by imbibition. *Ibid.*

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5. *New observations on the measure of the temperature of the organic tissues of the bodies of men and animals, by means of thermo-electric effects, by* MM. Becquerel and Breschet.—The memoir we now present to the academy is a succinct expositiou of the continuation of experiments undertaken by us at Paris, and in our journies to tho Alps and Italy, for determining, in a more rigorous manner than has yet been done, the temperature of the tissues in general, and of the interior organs of man and animals, by the assistanco of thermo-electric effects.

The use that we have made of tho mixed metallic needles, less than a millimetre in diameter, for determining the temperature of tho interior parts of organized bodies, require delicato precautions with which we have already made some persons acquainted, and without which it is not possiblo to obtain results, on the accuracy of which we can depend; we are now about completing what we have already said on this subject.

When ono of the extremities of a metallic bar is inserted into a source of heat, which is not capable of reacting chemically on its constituent parts, this bar becomes heated, by degrees, to a greater or less distance from the inserted part, according to the nature of tbe metal, the dimensions of the bar, tho temperature of the source, and that of the surrounding air.

Hence the different sections of the bar, above, and to a certain distance from, the source of heat, assume different temperatures higher than that of the surrounding atmosphere; but as soon as each of them attains the temperature it is to preserve, that is to say its state of equilibrium, experiment proves that for distances from the source which increase in arithmetical progression, the excesses of temperature decrease in geometrical progression, whenever the excess of the temperature of the bar over that of tbe surrounding medium does not exceed 20 or 30 degrees. On the other hand, the propagation of the beat varying according to the dimensions of the bar, the loss of heat being proportional to the area of the exterior surfaces, and the quantity of heat which traverses being also proportional to the area of the section, it follows, that the decrease of temperature will be as much greater as the circumference is less. Experiment effectually proves that in two bars of the same metal, not having the same transverse section, the distances of the focus from the points in which the temperature is tho same, are to one another as the square roots of their thicknesses, or as the square roots of their radii if the bars be cylinders. It follows from these different observations that the smaller tbe diameters of the cylinders or metallic needles, the less the source of heat would become cooled when its temperature would be capable of varying by the presence of these needles; hence the necessity of operating with needles whose diameters are less than a millimetre.

It also follows, from the preceding observations, that when we seek to determine the temperature of the interior parts of a man who is about 37 degrees, we must place him in a medium whose temperature is at least 18 or 20 degrees. If this condition do not yet suffice, we must find by previous experiments, the effects due to tbe cooling produced in tho muscles by the presence of the needles. This is a point on which, perhaps, we have not been sufficiently determined in onr preceding memoirs.

The process for finding the interior temperature of the human body consists, as is known, in making use of two needles each composed of two others, one of copper and the other of steel, soldered at one of their ends. One of them is placed in a medium whose temperature remains constant during the time of the experiment, whilst the other is introduced into the part the temperaturo of which we wish to measure. These two needles are connected on one side, by their

steel end, with a steel wire of the same nature, and on the other, by their copper end, with the extremities of tho wiro of an exceilent thermo-electric multiplier.

When the two soldered needles have tho same temperature there is no deviation of tho magnetic ones; hut for the least difference in the two temperatures, be it only 0·1 of n degree, there is a deviation whose direction and extent serve to estimate correctly this difference, and consequently the temperature of one of the media, when that of the other which is constant is known.

The constant source that we usually employ is furnished either by the apparatus of M. Sorel, which has already been described, or by the mouth of a person accustomed to this kind of experimenting. Sorel's apparatus preserves for some hours, a temperature, only varying a few tenths of a degree; hut the mass of water which gives it is so considerable, that the solder immersed therein is immediately put in equilibrium of temperature with it, notwithstanding the losses experienced by the parts of tho needle placed on the outside, which are quickly repaired In this case, the temperature shown by the solder is that of the medium in which it is found. It is not the same with the temperature shown hy the second solder, which is found in a muscle a smull distance from the skin, which muscle, hy reason of the tissues of which it is composed, from their small extent and bad conductibility, ought not to be considered as an equal source of heat to tho other; we also find when operating in a medium, whose temperature is below 18 or 20 degrees, a difference of the apparatus, even when the temperature of tho latter is the same as that of tho muscle.

By using the mouth as the source of constant heat, we have not to fear so much the differences that we have just shown, because the two sources have an analogy among themselves, with regard to their constitution.

We have entered into somo details on tho precautions to he taken, when we endeavour to measure the interior temperaturo of organized hodies, in order to enable those persons wishing to mako use of our procedures to avoid the indicated causes of error.

We shall now mention the experiments we havo made for showing how far the mouth may replace the apparatus for constant temperature.

Each of the solders was placed in tho mouth of a young man 22 years of age, between the palate and tho tongue, which exercised a slight pressure on the metallic wire, so as to avoid tho variations resulting from the passage of the air breathed. Tho magnetic needle deviated $1\frac{1}{2}$ degrees in favour of one of the two mouths The solders having been transferred from one mouth to the other, the deviation was 2 degrees in another direction, instead of $1\frac{1}{2}$ degree. The difference of half a degree, corresponding to one-tenth of a degree of temperature, proceeded very probahly from the solders not having been placed alike in the two experiments; the effects did not vary for a quarter of an hour.

Henco we see that with certain precautions wo can make use of the mouth as a sourco of constant temperature, when we are accustomed by previous attempts to keep tho solder always in the same position, and to breathe through the nose, so as not to introduce cold air into the mouth.

One of the solders having been placed in Sorel's apparatus marking 36 degrees, the other in tho mouth of a young man, the deviation of the magnetic needle was two degrees in favor of tho mouth, which indicated a temperature of 36° 40′ instead of 36° 50′, shown by the thermometer; a very slight difference owing to unseen causes.

The one solder was left in the mouth as it was, and the other was placed in the biceps muscle of tbe second young man, the temperature of the air being 14 degrees, consequently, helow what is necessary for the success of the experiments; we had n deviation of 4 degrees in favour of the month; hence, the temperature of the biceps given hy the needlo was only 36° 20′, instead of 36° 60′, which is the mean temperature wo have found in our preceding memoirs.

The solder which was in the mouth, was taken out to be placed in Sorel's apparatus, which showed 33° 50′ by the centigrade thermometer; the deviation of the magnetic needle was 10° in favour of the apparatus; hence, tho mouth possessed

a temperature of 36° 50′, as we have previously found it. Thus the mouth may be used with advantage as a source of constant temperature.

We have naturally been led to make some experiments on the influence of the variations of the surrounding temperature on that of the human muscles. This question, which has occupied philosophers and physiologists for some years past, is not yet completely resolved, wherefore the results that we have obtained will not be without interest for the science.

It is certain that man, as well as warm blooded animals, can live in an atmosphere which differs nearly 80 degrees in temperature from their own, since the inhabitants of the polar regions, covered it is true with clothes, are one part of the year exposed to a temperature at which mercury freezes. Hence, men as well as warm blooded animals possess in themselves the faculty of increasing in a given time the heat that they develop. As to the faculty which they have of resisting high temperatures, without any sensible disorder in the animal economy resulting, we shall refer to the experiments of Banks, Blagden, and Fordyce, who have remained exposed for some moments to a temperature of 125 degrees, without finding any sensible change in their own, estimated probably from that of the mouth.

On the other hand, Berger and De la Roche, having been exposed to a temperature of 49 degrees found theirs increased 4 degrees; and De la Roche, having remained alone in a hot-house at 90 degrees, for sixteen minutes, has proved that his was only increased 5 degrees.

Captain Parry relates, that in the polar regions, where the temperature is lower than that at which mercury freezes, that of man is not sensibly modified. This last observation is contradicted by Mr. John Davy and some others, who have found that the temperature of man increases from the poles to the equator.

Without wishing to enter into an examination of the contradictory results we have just mentioned, we shall confine ourselves to mentioning the experiments we have made on the same subject.

We introduced into the biceps muscle of the right arm of two young men, each of the solders of two perfectly similar needles; the temperature of the surrounding air was 16 degrees; the magnetic needle showed no appreciable deviation; hence, the two muscles had exactly the same temperature. One of the arms under experiment was immersed as far as the elbow, incessantly for a quarter of an hour into water, at 10, 8, 6 degrees, then at 0; the experiment lasted about an hour; the deviation of the magnetic needle was only two degrees in favour of unimmersed muscle, which indicates a diminution of temperature in the other of about the fifth of a degree.

The same arm having been afterwards plunged in water at 42 degrees for fifteen minutes, the temperature of the immersed muscle was only increased the fifth part of a degree.

These experiments having been repeated at different times, we have always found but very feeble differences in the temperature of the muscles.

These results have been confirmed by the experiments we have made at the mineral water baths at Lovech, in Valais, two years ago, and recently at Paris, with the assistance of M. Seguin, external pupil of the Hotel Dieu, at Paris, who assisted in our researches with a zeal worthy of praise. We were not contented with putting the arms in the water at an elevated temperature, but immersed the whole body therein. The waters of Lovech were 49° centigrade.

The temperature of Sorel's apparatus indicated 35° 50′: one of the solders was placed in it, while the other was introduced into the biceps muscles of M. Seguin; the deviation of the magnetic needle was 12 degrees in favour of the muscle, which indicated a temperature of 36° 70′. M. Seguin having been placed in the bath at 49 degrees, remained there twenty minutes; the deviation of the magnetic needlo varied from 12 to 13 or 14 degrees according as it was more or less distant from the water. Hence the temperature of the muscles increased from one to two fifths of a degree. On coming out of the bath the devi-

ation of the magnetic needle returned to 12 degrees as it was before. M. Seguin's pulse made 112 pulsations per minute in the bath.

We obtained the same result on a vigorously constituted young Tyrolean carpenter. We were unwilling to repeat the experiments at a higher temperature, for fear of injuring the health of persons volunteering to assist in our researches. But we have recommenced them at Paris at a temperature a little lower than 49 degrees, with the assistance of M. Seguin and M. Castille, also external pupil of the Hotel Dieu. One of the solders was placed in M. Costille's mouth, the temperature of which, measured by the thermometer, was 37° 50′, the other in the biceps muscle of M. Seguin's right arm; the deviation of the magnetic needle was 2 degrees in favour of the mouth, which indicated a temperature of 37° 10′ for the muscle. Mr. Seguin was placed in a bath at 42° 50′ and remained there twenty minutes; the temperature of the muscle was not changed, as the deviation of the magnetic needle remained the same.

This experiment repeated on M. Castille gave the same result. We see by the facts just mentioned, that when the human body is in contact with water, whose temperature varies from 0 to 49 degrees during a space of twenty minutes, that of the muscles experiencing only feeble variations, perhaps it would be the same if the contact were prolonged for some time, as the experiments of Mr. John Davy and other philosophers lead us to believe; but it is impossible to verify this assertion, since very serious disorders in the general economy would result from it: a bath of 49 degrees already strongly reddening the skin and determining the blood to the head.

We may also conclude from some observed facts, that the results obtained by M. De la Roche, who was placed in a hot-house at a temperature of 49 degrees, are due in a great measure to the phenomena of respiration, which modify the temperature of the mouth.

We shall also relate one experiment made at Lovech, and which has not been repeated on account of the difficulties it presents. This time it was a dog on which we experimented; his muscles indicated a temperature of 38° 50′; plunged in a bath at 49 degrees, the needle not touching the water, the temperature of the extensor muscle increased successively from half a degree to 1, 1½, and 2 degrees, and that in the space of five minutes. The dog became so furious that we were obliged to withdraw it from the water; after a short time the temperature of its muscle returned to what it was at first.

The solder was introduced into his chest, and we obtained equally an increase of temperature of several degrees some moments after the immersion in the bath: this increase took place chiefly when the animal was violently agitated. We are ignorant of what influence the exasperated state of the animal had on the effects that we have observed. We shall also mention a curious result, which has no relation to the preceding ones, but which will interest physiologists.

One of the solders was placed in the biceps of a young man, the other in the great supinator muscle of the left arm of a man 45 years old. The magnetic needle underwent no sensible deviation. The vein was opened, but we observed no change of temperature during, and after the loss of the blood. The solder was placed as near as possible to the vein. We may draw what conclusion we please from this fact; but the only one which appears natural to us, is that *a priori*, we ought to think that it would be thus, because the blood, whose escape was permitted by the opening of the vein, returned to the heart, and having already circulated through the capillary vessels, has become foreign to the composition of the tissues in returning to the central organ of the circulation by the branches and veinous trunks. Hence it could only produce a decrease of temperature in the animal body by its abundant flowing out, and producing a weakness of the subject. We therefore thought it right to make the experiment in another way; on which account we took a middling sized dog, which had eaten a few hours before the experiment, and placed one of the solders in the muscles of the fore part of the thigh, while the solder of another needle was in the mouth of an experimenter, a bandage having first been thrown round the femoral artery, im-

mediately below the outlet of the abdomen. The suspension of the blood's course in this vessel, caused no change in the temperature of the limb, and by several repetitions we exercised or suspended the compression on the arterial trunk, without being able to observe the least motion in the needle of the multiplier.

Would it be necessary, in conclusion, that the modifications in the temperature of the tissues, depend much less on the sanguinary circulation than on the nervous influx, or even that the results of this last experiment prove that, in only tying the femoral artery, we have not stopped the whole of the blood in the vessels of tho thigh, the gluteal and ischiatic arteries being able to make up for the femoral one.

In order to have a positive solution of this physiological difficulty, we have embraced the primitive iliac artery with a doublo silk cord; then placing one finger on the vessel at the point where the ligature bound the vessel, wo could at pleasure hinder or permit the circulation of the arterial blood in the limb. The needle was then inserted into the fleshy parts of the thigh, and at the end of eighteen minutes we perceived the temperature lower about half a degree. Afterwards, permitting the blood to traverse the arterial vessels of the thigh, the temperature was soon re-established in its normal state. This experiment repeated several times gave us the same result; although the effect observed be very feeble, it shows, nevertheless, that the arterial blood exercises a direct influence over the temperature of the tissues; it is not, however, to the blood which circulates in the trunks and arterial branches that we must attribute this influence; but that which traverses the capillary plexus. In fact fifteen or twenty minutes usually elapse between the suspension of tho blood's course in the limb, and the diminution of the temperature. However, tho re-establishment of the temperature in its normal degree, when the blood is permitted to traverse the arteries, was always more rapid than the diminution of temperature when the trunk of the principal vessel was compressed.

We have here stated what relates to tho influence of the arterial circulation over the temperature of the animal tissues; in another memoir we shall mention what experiment has taught us of the nervous influence, with regard to this samo temperature of the tissues.

The facts that wo have just related in this memoir, show anew what we may deduce from the thermo-electric effects, to estimate the temperature of the interior parts of man and animals; taking as a constant temperature either that of Sorel's apparatus or that of the mouth of a person accustomed to this sort of experimenting.—*Annals of Electricity, &c.* June, 1838. From *Compte Rendu des Séances de l'Académie des Sciences.* April 9, 1838.

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6. *Temperature of the human body in different degrees of external heat.*—During the voyage of "La Bonite," for the purpose of making various scientific investigations, the attention of the naturalists was especially directed by the Academy to determine the temperature of man and animals in different parts of the globe. Experiments were therefore made on ten men, every day, from April, 1836, while the Bonite was at Rio Janeiro, till her arrival in France, in November, 1837, by introducing a thermometer into the rectum, and observing carefully the temperature of the surrounding atmosphere. All the men were occupied in a constant employment on board the ship, with regular diet, &c., so that no interfering agency could be present. Tho results obtained are, that the temperature of the human body falls very slowly during the passage from hot into cold regions; and that, on the contrary, it rises more rapidly in passing from cold countries to the torrid zone. The degree of change varied in different individuals. The average temperature of the men, observed at Cape Horn, at 59° south latitude, with an external atmospheric temperature of 0° centigrade, presents only a difference of one degree from the average observed in the same men in the Ganges, near Calcutta, with an external temperature of 40° centigrade. Thus a variation of forty degrees in external temperature gives rise to a difference of only one degree in the temperature of the human body.—*London Med. Gaz.*

May, 1838. From *Rapport sur les résultats scientifiques du Voyage de la Bonite autour du monde*, par M. de Blainville.

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7. *Excito-motory system.*—The following report of a discussion in the Physical Society, Guy's Hospital, will be read with interest, as it exhibits the views of some distinguished physiologists relative to the excito-motory system, a subject at present exciting much attention:

"Mr. Grainger stated that anatomists, up to the present day, had been in the habit of describing the spinal nerves to arise on each side by a double origin. The posterior or sentient root and the anterior presiding over motion. To these M. Hall has added another pair of roots, which he has termed respectively the incident and the reflex; the former proceeding with the sentient root and serving to convey impressions (independent of sensation) to the latter, which accompanies the motor root, and constitutes the conductor of involuntary motion.

"Sir Charles Bell's discoveries had shown that every nervous filament possessed but a single faculty, and wherever a nerve appeared to have two functions, it was necessarily made up of two sets of fibres. Dr. Hall's experiments had demonstrated, that when the brain was completely separated from the spinal cord, voluntary motion and sensation in the body were completely arrested; but upon the application of some irritation to the surface, the muscles could still be made to contract, independent both of sensation and of volition. Hence the Doctor reasoned that some chain of connection existed between the nerves of motion and perception in the spinal cord, altogether irrespective of the brain, and such as have been already alluded to. Mr. Grainger had endeavoured to verify this theory by actual dissection, and had satisfactorily demonstrated the fact, that such connection did indeed exist: for he found each root, as it arose from the spinal marrow—the anterior from the motor tract, and the posterior from the sensitive—to have also another origin deeper in the cord, from the central gray matter; whilst the original roots proceeded no further than the white medullary structure of the cord; the posterior of these origins, from the gray matter being the incident nerve, and the anterior the reflex nerve, of Dr. Hall. Thus, like the discoveries of Bell, had the more recent theory of Hall been verified by dissection.

"The idea, indeed, was not altogether original on his part, for it had been already mentioned by Gall, Bellingeri, and Mayo, that the nerves of the spine arose not from the medullary alone, but also from the cineritious part of its structure. This had not, however, been fully admitted by anatomists; and although many investigations on the subject had been made, he was not aware that the fact had been previously satisfactorily demonstrated. He was happy to add, that his dissections had been now confirmed by those of Mr. Solly.

"If these notions were correct, the quantity of brain and white part of spinal cord ought to bear a proportion to the development of sensation and volition; and that of the cineritious part should be in proportion to the development of motion and perception. And so indeed it was, this being fully verified by comparative anatomy, which showed, that in man the brain and white part of the cord preponderated largely, whilst in animals endowed with strong motive powers the cineritious part of the spinal marrow prevailed.

"Mr. Grainger illustrated these facts by reference to examples from fishes: as the Lophius piscatorius, and from the lower classes of the animal kingdom; as the Annelidæ, the common earth-worm, insects, &c., by which it entirely appeared that the persistence of muscular movement bore a decided ratio to the development of the spinal and ganglionic systems. Mr. G. concluded his remarks by expressing his conviction, that dissections in anatomy had hitherto been too coarse, and the recent labours of Kiernan, Müller, and others, gave encouragement to hope that many parts of anatomical structure were susceptible of far minuter demonstration than as yet had been applied to them.

"Dr. Marshall Hall rose in explanation of his views. In the cerebro-spinal system there were, indeed, two systems—the cerebral, containing the nerves of volition and sensation, and the spinal, which operated independently of either

to produce motion. If the brain of an animal be removed, though motion may occur, yet such motion cannot be the result of volition, nor can any motion whatever be produced without the application of some external irritation. Thus, if a snake be decapitated, and then left till it he perfectly quiescent, though it may be made to move by external stimuli, yet if left to itself no motion will bo observed again, and it will remain in whatever form it may have been placed. The movements which take place upon the application of external stimuli in the coma of apoplexy, opium, &c. &c., also in the case of an animal struck down by a blow on its head, indicated that there existed some other source of contractile power besides that of the brain, and independent both of volition and sensation. Reverting now to the paper, Dr. Hall said, that experiments enough were on record to show, however much you may lacerate or tear the cerebrum, no convulsive movements will arise. It was a matter of great interest, therefore, to inquire why convulsions occurred in certain inflammatory and othe rconditions of that organ. With regard to the cerebellum, Serres, in speaking of an apoplexy there, and believing it to preside over the sexual passions, had prognosticated the disease from the existence of priapism. Doctor Hall did not believe this to be the function of the cerebellum: he thought the priapism might be explained upon the supposition that tho clot pressed upon and irritated the adjacent medulla oblongata, the source of muscular movement—as in the guinea-pig, irritation of that part immediately produced a seminal emission. To apply this physiology of tho excito-motory nerves, the Doctor adduced the case of the crowing inspiration of infants, which always depended upon one of three causes—dentition, constipation, crudities in tho stomach. Removing the cause of irritation invariably relieved the crowing; in this affection the brain being only secondarily affected, and becoming diseased finally, because every convulsion renewed congestion there, and this ultimately gave rise to disease.

"Mr. Brereton agreed with Dr. Hall in some points, but differed with him in others. He was not satisfied that sensation was not essential to the production of motion, for although the hrain be removed, it had not yet been proved that sensation did not also reside in the spinal cord. In several cases of paraplegia, and various forms of palsy, in the hospital, he had lately endeavoured to excite motion by tickling, pinching, stabbing with a pen-knifo the affected limb, &c. &c., but he had altogether failed in producing the result stated by Dr. Hall. An acephalous fœtus had been known to cry; this Mr. Brereton did not comprehend, for crying presupposes a sensation of something unpleasant, a certain exercise of the will. He agreed in tho opinion that the cerebellum did not preside over tho sexual appetites. With regard to the crowing of children, Dr. Hall had mentioned but three causes. Dr. Hugh Ley, however, has described a fourth, viz. tumours in the neck pressing upon the par vagum.

"Dr. Hall replied, he thought there must be some error of observation in the experiments made by Mr. Brereton upon the paralytic patients, for it was quite proved that convulsion of a paralysed limb could be produced in the way he had stated. There was now a patient in the 'Dreadnought,' under Dr. Budd, completely paralysed; tickling the soles threw the limbs into strong movements, and passing the catheter occasioned priapism. With regard to the acephalous fœtus, there is but one case of crying recorded; it is by Ollivier, and the word used is '*crier*.' This does not necessarily signify crying; it may mean a hiccup, or any noise in the air-passages. He differed altogether with Dr. Hugh Ley about the crowing in children, and did not believe that pressure on the pneumogastric nerves could occasion crowing; any pressure there caused stupefaction, as in excessive drunkenness, coma, dyspnœa, &c.; and in a case of the kind at the Salisbury Infirmary, Mr. Sampson had saved life by a well-judged operation of tracheotomy.

"Mr. Aston Key suggested whether, in the case of paralysis where Mr. Brereton had failed in exciting motion, the injury or disease of the cord had been low down, at the end nearly of all the cineritious matter: might not this be sufficient to account for the failure?

"Mr. B. Cooper said it was the same thing physiologically, whether the

injury was in the spinal cord or in the cauda equina; which depended upon and arose from the spinal cord.

"Mr. Brereton.—One patient had fracture of dorsal vertebra. A catheter was always in the bladder, but there was no priapism, and no movement was caused by pinching or blistering. Another had injury just below the head: here, too, he had failed in his experiments. On looking over the old museum-books of Guy's, Mr. B. had found there was once a patient, under Mr. Morgan, with injury at the sixth cervical vertebra. There was at first (as is usual) paralysis of tho parts below the seat of injury. After a time, however, (some days,) the arms also gradually lost all power. He would ask Dr. Hall whether he knew of other cases where disease in this way seemed to affect parts above the seat of injury; and whether any analogous result could be produced in experiment by the agency of the excito-motory nerves.

"Mr. Golding Bird stated, that ho had electrified many paralysed patients in the hospital, and, in every instance, contortions of the limbs were produced by the electricity.

"Dr. Hall had no information upon the question last put by Mr. Brereton: it was extremely interesting, and he had often thought of it; but he had never seen, or before heard of, any case bearing upon it. Mr. Bird's fact about electric fluid proved nothing; for electricity. or galvanism, acts directly on the muscular fibre, and not through the agency of the nerves.

"Dr. Bright expressed his gratification at the very complete manner in which the excito-motory theory had been verified by dissection. The Doctor related a case which he had seen with Mr. A. Key, where perfect paralysis of the lower extremities existed, and in which violent convulsive movements could be produced by tickling tho foot. Ho believed some explanation might be found for the failures experienced by Mr. Brereton: he did not think they were to be looked upon as militating against the excito-motory theory.

"Mr. Grainger said that he believed Mr. Brereton's failures might be accounted for by the fact that the excito-motory influence was differently diffused (as regarded intensity) in different parts of the limbs; nature, who gave nothing in vain, had not afforded the excito-motory influence to parts that did not stand in need of it. Thus, though many parts of the leg were incapable of rendering involuntary contraction from irritation, yet the sole of the foot would at once yield the phenomenon. Mr. Verrall had made experiments upon a case under his care, showing this very completely. Mr. Grainger concluded by adducing illustrations on this point from comparative anatomy."

At the subsequent meeting of the society, Mr. Molloy stated, in reference to a case in the Dreadnought, quoted by Dr. M. Hall, that since the last meeting, he had visited the patient "in company with Mr. Edge, and had found that complete paralysis did not exist; for motion and sensation, to a limited degree, remained. In another case which had also been quoted, although there was complete paralysis of motion, yet sensation was not destroyed.

"Mr. Hilton said that his dissections did not coincide with those of Mr. Grainger. Mr. Grainger had described both the anterior and posterior roots of the spinal nerves to arise from the *middle lateral* column of the spinal marrow, and not at all either from the anterior and posterior columns. Mr. Hilton wished to reservo a minute statement of his own observations for another occasion, and would at present content himself with merely saying that he had traced both to the anterior and posterior columns. With regard to the excito-motory experiments of Dr. Hall and Mr. Grainger, there existed, he thought, one source of error—viz. that the sympathetic system of nerves had never been removed from the sphere of operation; and it was therefore difficult to say how much of the results obtained by these gentlemen might be assigned to these nerves. For the last six years, he (Mr. Hilton) had been in the habit of describing, in his public demonstrations of the nervous system, at this school, the various muscles of the body, and the integument immediately covering those muscles, as being supplied by the same set of nervous filaments. So that when an irritation was applied to the surface by means of those nerves, it immediately excited the muscles con-

nected with that part of the skin to contract. This was done instantaneously, and for an appreciable moment of time before the exercise of volition.

"He thought that pathology generally bore out the views of Foville, who connected the thalami with the upper, and the corpora striata with the lower, extremities of the opposite side; and the difficulty which appeared occasionally to occur, seemed to him to be often explicable, as follows:—The nervous power is conveyed from both these bodies to the respective limbs downwards, along the crura cerebri. Now if the lesion existed in the thalamus, or in the crus cerebri below it, the chain of connection between the corpus striatum and the lower extremity was cut off as effectually as if the lesion had been seated in that body itself.

"Dr. Whiting said, there were abundant facts in pathology and in physiology to show that muscular contractions could occur independent of volition—as in tetanus, sneezing, coughing, &c. But many paralysed cases had fallen under his observation, in which the excito-motory influence, as described by Dr. Hall, could not be made to operate.

"Dr. Marshall Hall said that one positive fact was of more value than a thousand negative ones, and that the latter by no means invalidated the general rule. The failure of excito-motory influence in these negative cases, might, in some instances, be accounted for by the circumstance of some disease or failure in the nerve between the spine and the irritated surface. Several years ago he had noted the fact, which he could not then explain, that if a frog be decapitated, and its spinal cord be afterwards divided in the middle of the back, upon irritating the posterior extremity it became immediately convulsed. Not so in the toad, treated in a like manner. He now knew, by the aid of anatomy, that in the latter the cauda equina commences very high in the back, and was divided in the experiment. In the former, the spinal cord was cut through.

"Mr. Key supported the view taken by Dr. Hall, and quoted cases in which paralysed limbs moved powerfully when touched or irritated."—*London Medical Gazette*, April, 1838.

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8. *Case of Partial Ectopia Cordis and Umbilical Hernia.*—In our No. for February 1833, will be found an interesting account, by Dr. Thomas Robinson, an eminent physician of Petersburgh, Virginia, of an infant in whom a portion of the anterior parietes of the thorax and abdomen were deficient, by which the heart was exposed to examination both by sight and touch, and its actions could be inspected. A similar case has recently been communicated to the Provincial Medical and Surgical Association, by Dr. John O'Brien, of Bristol, and is published in the sixth volume of their Transactions.

This case is so important in its bearings on some disputed points in physiology, that we give the details of it in full. It will be observed, that the observations of Dr. O'Brien are entirely confirmatory in several important particulars of those of Dr. Robinson—as respects the active dilating power of the heart, the suddenness of its systole, &c.—and in opposition to the views of most physiologists.

"A child, æt. 14 days, presented the following appearances:—She is healthy, large, and was born at full term; colour of the face and skin perfectly natural; she takes the breast well, and sleeps quietly. The secretions and excretions are normal. The head is raised from the chest at each systole of the heart, which occurs 140 times per minute; inspirations 45 per minute whilst the little patient is asleep; the dyspnœa is much lessened when she lies on her back with her head on a level with her body. The shape and outward form of the thorax is perfect, with the exception of the greater part, if not the whole, of the ensiform cartilage, which is wanting. The functions of the cerebro-spinal system is apparently normal.

"At the anterior and superior part of the abdomen, between where the umbilicus and the lower end of the sternum ought to be, exists a tumour, soft, oval, unequal, and semi-transparent, three inches and a half in length, two and a quarter in breadth, and one and a half (at a medium) above the level of the pari-

etes. The inferior three quarters of this tumour is evidently occupied by tho floating viscera, which have escaped for want of the support of the linea alba, and of the oblique, the transverse and recti muscles, the superior portion only of the last being I think wanting. The skin covering this inferior section of the tumour is reddish and shiuing, being evidently of late formation; on the left side of it is an ulceration about the size of a half-crown, where the cord was inserted. Around the base of the tumour, particularly the superior portion, where the integuments of the hody meet those of the hernia, there is a raphé, which, with the appearance of the skin, shows that the abdominal cavity remained open to a late period of utero-gestation.

"The superior quarter of the tumour has a triangular shape, bounded laterally by the cartilages of the false ribs, and inferiorly by what appears to be the transverse colon. In this triangle, which is exactly in the median line, is seen through tho diaphanous skin, a hody pulsating in shape and appearance not unlike a small heart, with its point directed outwards, thus forming nearly a right angle with the sternum, its apex being pushed upwards by the distended colon; but when the intestines are not so distended, the angle becomes a very obtuse one.

"The blood-vessels ramifying on this body were easily recognised through the delicate and almost transparent skin, which became injected and of a dusky tinge whenever the infant forced down or retained her breath. Three distinct motions or actions were evident, I believe, to almost every person who examined the tumour, and they were not a few, and amongst them Dr. Charles Williams of London.

"First.—A lessening in size and a contracting of its whole body one hundred and forty times per minute, during which a dimple was formed on its side, varying io depth according as it emptied itself of the whole or only a part of its contents; the depth was always increased when the infant took a deep inspiratioo and was very quiet, as in sleep. This contracting or systole commenced suddenly, and diminished considerably the size of the body; after repeated observation, ond the most attentive examination, this first motion appeared to be synchronous with the pulse in the carotid, and with the first or ventricular sound.

"Second movement—or that of dilatation, during which its body becamo tense, and appeared shortened, while, at the same time, it was much enlarged by as active a force as that of contraction, (it was dilated even when, by *pressure*, we attempted to prevent it,) whilst in the fingers, it gave me, as well as many of my medical brethren, a sensation as if it were first *forcibly* enlarged, and that then a fluid rushed in, with one wave, communicating the feeling of a thrill. The dilatation was synchronous with the second or loud *sound*, but it appeared *to continue after it.*

"During the systole, tho third or downward movement of tho whole tumour was observed to take place, (it certainly commenced rather before than after the systole,) evidently distinct from that caused by irregular periods, by the coutraction of the diaphragm, as well as hy deep inspiration. To make this motion more evident, I pushed the pulsating body into the thorax, where it required a considerable force to retaiu it, as *during each* systole it was forced dnwn against my fingers, pushing them forwards, and this with a more equal power each time, when tho pulse was regular and full, than when it beat ono strong, followed by two or three small pulsations; the same was observed to take place in the tumour, and I thiuk this is easily expluined, by supposing that the ventricle emptied itself during tho first, and ouly partially *during the three* succeeding pulsations.

"From the loud noise, or that caused by the reaction of the arteries on tho blood expauding the semilunar valve, to *the duller* or that called ventricular, the space of time appeared to be about one holf of the whole time of the heart's actioo, if any thing, rather more, as observed by the eye, but the movements were so quick that I sholl not attempt to advance any thing positively as to the exact quantity of time occupied by each motion separately; the period of rest was all but imperceptible, indeed it appeared iuseparable from the dilating, but

more especially the filling of the ventricles, or that period when the thrill was felt.

"Taking the tumour in the fingers of one hand, and passing those of the other under and behind it, they came into contact with a large round body within the thorax, (the skin was so lax, it permitted this to be done with facility,) whose pulsations were synchronous with those of the tumoor. This same body was also felt in front, and might have heen mistaken for the pulmonary artery.

"Haodling the tumour, or touching the body within the thorax, did not appear to give rise to the slightest sensation on the part of the littlo patient, in this agreeing with the case of the celebrated Harvey. There was evidently no hernia of the abdominal viscera *into the thorax*, and *vice versa:* nor, on the other hand, was there any hernia of the thoracic viscera into the abdomen.

"The chest sounded well, heing clear over that spot where the impulse is generally felt, hut I was prevented by circumstances, viz: the age, the dyspnœa, &c., from deriving more accurate information from this source of diagnosis. The respiration was natural for an infant, and evideot in the precordial region, showing that a portion of lung occupied that region. The sounds of the heart were clear and distinct in the precordia, rather anteriorly; but they were evident over the whole thorax, accompanied by *no impulse*, or any abdominal noise.

"In the lower portion of the abdominal tumour, which became mnch distended whenever tho child cried or forced downwards, the vermicular action of the small intestines was very distinct.

"Sept. 28. The pulsating hody has increased in size, and the skin covering the tumour is quite white; the ulceration entirely healed; the patient has had one convulsion.

"Oct. 1. Tho infant has taken cold, and become mnch emaciated tho last two days. Convulsions occur more frequently to-day, pulse not to he counted. respiration increased to 53 per minute, a general mucous rattlo over the chest; the colour of the face remains unchanged, though expressive of great anxiety.

"5. The lips have hecome slightly blue, she is apparently sinking, has had two convulsions to-day, and vomited somo matter streaked with florid blood. She died a few hours after without a struggle, being then three months old.

"*Post-mortem examination two days after death.*—An incision was made through the skin from tho top of the sternum to the puhes; whilo dissecting back the skin, not a trace of a muscular fibre conld be discovered over the superior part of the tumour, neither the recti, the oblique or transversalis muscles, nor the linea alba. The transverse colon appeared the instant the skin was divided. forming the hase of the triangle described in the history of the case; the cartilages of the ribs were perfect; the sternum was perhaps a little shorter than natural; and the ensiform cartilage was entirely wanting. The liver was very large even for an infant three months old, extending quite across the abdomen: with this exception, all below the diaphragm was normal. This muscle was itself normal, with the exception of the band or bundle of muscular fibres which attaches it to the ensiform cartilage. Its usnal attachment to the posterior face of the cartilages of the false ribs continued, as is natural, hut the ensiform cartilage heing absent, it passed from one cartilage to the opposite one without its proper support in this place. The consequence of this was, that a triangular opening, formed laterally hy the cartilages, and inferiorly by tho falling and floating portion of the diaphragm, remained, close to that spot where the pericardium adheres to that muscle, and to the anterior mediastinum in front.

"The sternum being now raised, we discovered the heart in the pericardium nearly in its natural position, rather towards the right, its base occupying the left side of the thorax, and overlapped by tho lung. The right ventricle was hypertrophied, being double the thickness of the left, withs ome dilatatioo, and its apex was directed to the right side. The left was of its ordinary thickness, lying from left to right, and prolonged for abont ono inch and three quarters, into a sac formed of the pericardium, which with the sac protruded through the triangle above described, the prolonged portion forming, when in place, an obtuse angle with the remainder of the ventricle. The apex of the right ventricle pre-

vented the left coming further out. When we opened the pericardium, we observed that it was attached by old adhesions to the protruded portion of the ventricle. The anatomical formation of the heart was normal; the blood was fluid, and the heart contained no clot; the substance of the lungs was healthy and well inflated, and the hypertrophy of the right ventricle explained the congestion of the bronchial mucous membrane as well as the expectorations of fluid blood.

"It appears to me that the following conclusions may be drawn from this very interesting observation:—

"1st. It seems probable that the prolongation of the left ventricle was caused in consequence of the pre-existence of the triangular opening, as the action of the heart continually tended to force it against and through the aperture, and that the adhesions retained it there.

"2ndly. That in the production of the impulse, no account has hitherto been taken of the downward motion of the heart, produced, as I believe it to be, by two causes. The first of these is the sudden rush of blood from the distended auricle into the dilated ventricle sufficient to fill it, which must produce some degree of downward impulse to the heart; but if M. Bouilland's opinion of the injecting powers of the auricles be correct, then it must be of some amount. The second is the recoil or rebounding force of the heart when the ventricles have driven a column of blood into the aorta and pulmonary artery. Unite these two forces, and I believe they tend to increase, if not partly to produce, the impulse.

"Let us see if pathology does not bear out this view. When the ventriculo-arterial orifices are obstructed, or when there is hypertrophy, either eccentric or concentric, the impulse is increased in proportion to the obstruction and to the power of the muscle, the rebound being equal to the force exerted by the ventricles to expel the column of blood. Does not this solve the question of increased impulse; and that, too, in proportion to the disease? The received opinion of the present day is, that the impulse is caused simply by the systole straightening the anterior convexity of the ventricles, and thus bringing the apex into forcible contact with the ribs. It seems to me, if to this be added the above two forces, the impulse, or rather its cause, would be better explained. Perhaps also the *direction* in which these forces act might still more *perfectly* explain it.

"3dly. That the dilatation of the ventricles is as active a force as the contraction. Dr. Copeland supported this opinion many years since, and still, I believe, adheres to it.

"4thly. That dilatation is the cause of the gush of blood from the auricles, not its effect; that acting, as in this case it appeared to do, upon the principle of the common pump, it tended to carry on and explain the circulation in the large veins and through their valves, to extend the effect of the same principle to their minute divisions. Hamberger and Dr. Copeland fully concur in the first part of the above conclusion, and M. Bouillaud's opinion nearly agrees with this inference, only that he attributes an injecting power to the auricles.

"5thly. That no sound was produced by the contraction of that portion of the left ventricle, isolated as it was from the remainder of the heart, the sounds appearing to proceed from the neighbourhood of the valves. I merely here state what were the ideas excited in me and in many of my medical brethren who saw the little patient, after very frequent and most attentive examination. This conclusion is, I know, in contradiction to that come to by the Committee of the British Association, who decided that the first sound is caused by the muscular contraction of the ventricles. If this were the case, is it not probable that this isolated portion of the ventricle would have caused some sound? When taken in the fingers, and even held alternately under the stethoscope, and to the ear, a transmitted sound was heard, but no direct one, except that caused by the friction of the body against the instrument."